

Ten Research Questions

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The Financial Access Initiative is a research center based at New York University, focused on finding answers to how financial sectors can better meet the needs of poor households.

High quality evidence on the state of financial access around the world is advancing rapidly, as the chapters of this book illustrate.¹ A happy consequence of increasing knowledge is the ability to better recognize what we don't yet know. Here are ten questions, some micro, some macro, that need answers if we are to make informed decisions on how to improve financial access.

1. Does financial access—evaluated in typical settings with a long enough time horizon to see change—substantially improve the well-being of customers?

The most fundamental, unresolved question concerns impact. Does expanding financial access really make a notable difference to families and communities? And, if so, how and when?

Muhammad Yunus (1999) and other early microcredit advocates took us down a narrow path. Yunus's stress on "microcredit for micro-enterprise" continues to play well with the public, but we're learning that the rhetoric does an injustice to the complicated reality of how low-income families actually use financial services. The notion that microcredit loans are sought exclusively for business investment fails in the data, whether when asked directly (Collins et al 2009) or when derived indirectly (Karlan and Zinman 2011).

That takes us to the question of what borrowers are in fact doing with their loans when they're not funding business. One important use is to pay for big, lumpy expenses, including healthcare costs, school fees, and home repair. In a survey of customers of Mongolia's XacBank, for example, Attanasio et al (2011) find that about half of all microcredit business loans were used for household ends, not for business investment. Leading non-business uses include purchases of household assets such as video players, radios and large domestic appliances. A second type of use is to pay down more expensive loans. A third type is to help smooth

1. I appreciate editorial input from Timothy Ogden. The essay is forthcoming in *Banking the World* (MIT Press 2012), edited by Robert Cull, Asli Demirgüç-Kunt and Jonathan Morduch.

seasonal ups and downs of consumption. The uses make perfect sense from the standpoint of economic theory, but they make microcredit advocates nervous.

Advocates worry that borrowers cannot repay loans if there's no major business investment in the picture, but they miss the larger picture. The evidence in Collins et al (2009) suggests that borrowers repay microcredit loans with money earned in various wage and self-employment activities. Even if a particular loan is not used to *fund* self-employment, income from self-employment may nevertheless provide an important way to repay the loan. The distinction too often gets lost. From the bank's perspective, the central question should be whether the household can generate the cash flow to service the loan (and whether it will be in a position to apply that cash flow to loan repayments), not whether the loan is used for a particular purpose. The XacBank customers described in Attanasio et al (2011) still repay their loans, even when the loans are not used to finance business. Grameen Bank, for its part, reports loan repayment rates close to 100% even though at least one survey from Grameen suggests that many loans do not go to business purposes (in a small sample collected by Stuart Rutherford, about half of loans are used for non-business purposes; Collins et al 2009).

Given the complexity of what access to finance means for households, decent evaluations with clear bottom lines are needed to anchor conversations. But so far, evaluations have done little to settle the question of whether microfinance "works" or not. We have an older set of studies that hoped to measure the impact of microfinance on consumption or income but which were compromised in one way or another, usually by not making comparisons to credible control groups (Armendáriz and Morduch 2010 provide an overview and Roodman and Morduch 2011 demonstrate the lack of robustness of the well-known estimation of Pitt and Khandker 1998).

As control groups have become more credible, evidence of net impact has become weaker. Coleman's (2006) work in Northeast Thailand shows how this works. He finds that microfinance borrowers are already much wealthier than their neighbors before they gain access to microfinance. These wealthier households tend to see gains from access to microfinance, but their less wealthy neighbors do not on average. The overall finding is of no significant average impacts (Coleman 1999).

The push for randomized trials reflects the sense that they do far better in terms of credibility, but researchers are often forced to grab opportu-

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nities where they arise and thus tend to investigate narrow populations and short-term outcomes. One thing we have learned from randomized trials is that internal validity is crucial. We've also learned that internal validity is no substitute for external validity (Cartwright 2007), and it is questions around how—and if—results extend to other contexts that leave the literature unsettled.

Truth be told, however, the most unsettling finding is the lack of strong positive results. Neither the randomized trial completed by Banerjee et al (2009) in Hyderabad nor by Crépon et al in Morocco (2011), for example, find much in the way of strong impacts. The most positive finding so far is that of Attanasio et al (2011) in their 18-month study of XacBank in Mongolia, though, even here the story is mixed. Attanasio et al. find that access to microcredit group loans increased food consumption (and increased the quality of foods consumed as well). The result comes from both greater home production and from more spending. The effect is large: total food consumption in treatment villages was 17 percentage points higher than in control villages. Yet no such increase is found for households that receive microcredit loans using an individual-loan method (rather than a group method with joint liability). Nor does the early research find evidence of a parallel increase in income, even for households borrowing under the group lending method.²

Taking a structural approach, Kaboski and Townsend find in Thailand that credit constraints are rife and that microcredit access raises consumption. But when costs are compared to benefits, the costs of running the programs are 30% higher than the cumulative costs. Studies like these are forcing most academics to revise downward their expectations for microcredit impacts, but these are still early days in the evaluation literature.³

2. How much does consumption smoothing contribute to the welfare of families?

There are clear theoretical linkages between consumption smoothing, financial access, and improved wellbeing. Modern economics is built around the premise that households seek to maximize utility, not income. A core economic task of a household, rich or poor, is matching the availability of resources with the timing of consumption needs. This task is especially burdensome for poor households who have to piece together uneven cash flows using a handful of imperfect financial tools. A key role of access to predictable, reliable and convenient financial services is thus to smooth consumption (Collins et al 2009).

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2. As I write this the Attanasio et al paper is just being circulated in draft form, and it will surely generate much interest, discussion and clarification. If the past is a guide, there will be questions about the relatively short length of exposure to the treatment and the small sample (just 40 villages: 10 in the control group, 15 with group lending, and 15 with access to an individual lending methodology). The small sample size is on the low end of what is usually accepted for levels of statistical power in experimental designs.

3. While the new evaluation literature focuses rightly on study design, a complementary concern rests with data accuracy. The work of Cull and Scott in chapter 4 and Collins in Chapter 5 point to important practical steps for collecting sharper data on financial variables—especially with regard to informal mechanisms and the broad range of “semi-formal” options. Together with others, including Samphantharak and Townsend (2010) and de Mel et al (2009), the studies give us a better grounding for mapping theory and data. Improved data on cash flows, microenterprise finance, and the use of financial services will be one part of answering the impact question.

All the same, the links are not well-established empirically. People with more assets appear better able to smooth consumption, but it is unclear how much of that is self-insurance, how much is help from neighbors and relatives, and what the costs of the strategies are. Households can go to great lengths to smooth consumption, often using expensive or risky alternatives (such as moneylenders or asking neighbors to hold cash), but we don't have good measures of how much consumption smoothing affects household welfare. Even if average consumption over the year is unchanged, welfare can rise substantially if its distribution improves within the year. At present, we have little handle on relative magnitudes.

As to the reverse chain of causation, we have much further to go in describing the path from consumption smoothing to asset-holding and profitability. We have bits of evidence (e.g., Samphantarak and Townsend 2010 for Thai data), and need more on the ways that having a stable, predictable, reliable financial life carries over to investment choices.

The most interesting recent work is by Cole, Giné and Vickrey (2011) who extend their work on rainfall insurance in south India to determine the impact of access to insurance on production choices. They ask whether farm households with insurance will shift toward riskier (and more profitable) crops once insurance is available. They find a large and positive effect, with the share of households planting a (relatively risky) cash crop rising to 55% from 48.6% in the control group. The size is relatively large given the nature of the intervention, though it's unlikely to be transformative in itself. Still, the result stands as the most convincing link so far between financial access, risk reduction, and more profitable production choices for low-income households.

3. Why do so many micro-businesses stay micro?

In the large microfinance markets of Asia, a common but seldom-discussed observation is that the microenterprises nominally tied to microcredit borrowing rarely grow substantially, especially after the first few years. There are many possible reasons to explain this, including borrowers' simple lack of imagination, lack of management capacity, low profitability at scale, limited ability to hire trusted workers, risk aversion, lack of access to sufficient capital for productive growth investments, poor policy environments, and insufficient access to larger markets.

What role do financial institutions play? Making microcredit loans more flexible may help—though microfinance institutions worry that being

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more flexible may increase risk and costs.³ Field et al (2009) study 169 microcredit groups in West Bengal, India. Of these, 84 were allowed a 2-month grace period before loan repayments started. Those given the grace-period were twice as likely to start a new business, but the treatment group was also substantially more likely to default: 4 months after the final installment was due, 11% of the treatment group had failed to repay in full relative to 3% of the control group. (It's unclear how much the result reflects the impact of effectively increasing loan sizes, rather than adding flexibility *per se*.)

The lack of growth may also be due to competing household needs like childcare. Even if financial access makes a big impact at first, the long-run impact hinges on the extent of continuing gains.

4. Is SME finance an alternative strategy to microfinance?

If micro-businesses tend to stay micro, are there perhaps better options? Critics of the hoopla around microcredit suggest that job creation is better done by larger enterprises (e.g., Karnani 2007). The rush to support small and medium enterprises (SMEs) has been given attention by the G-20 countries and is tied in part to the idea that SMEs can contribute to the goal of poverty reduction by employing low-skilled workers. But can they? It's an empirical question which has been met with little evidence so far.

One piece of data comes from McKenzie and Woodruff (2008) who use data on tens of thousands of Mexican microenterprises to find marginal returns to capital of 15% per month for investment levels below \$200 and weak evidence of scale economies when investments get up to about \$1,000–\$2,000. Their overall finding is that there are no non-convexities in returns to capital of the kind that would suggest that bigger is better and that microenterprises are trapped in low-level equilibria.

Bauchet and Morduch (2011) approach the question not by looking at returns but by considering the structure of employment. They investigate data on the employees of SMEs supported by BRAC Bank in Bangladesh. Their conclusion is that these employees are far more educated and skilled than microcredit borrowers; in line with this, SME employees come from households that are considerably less poor on average. The average employee of a small enterprise in the BRAC Bank sample is a semi-skilled 26 year old male with almost five years of formal education. Bangladeshi microcredit borrowers, in contrast, are mostly women and about half have

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no formal education and most have few professional skills. Bauchet and Morduch (2011) find that just 7% of SME employees in their sample are men.

In sum, the two groups—SME employees and microcredit borrowers—look very different in the Bangladesh surveys. Will these kinds of results hold up elsewhere, particularly in Latin America and Eastern Europe where the gender and education profiles of microcredit borrowers is different from that in South Asia?

More important: if SMEs won't generate much poverty reduction through direct employment, can they create enough a difference by spurring regional demand and broadening the base of economic growth?

5. Which financial services are most valuable to the poorest?

Credit is just one useful financial service, but credit has been the first focus of microfinance institutions because there's a business model that makes lending possible, not because it is necessarily most important for customers. Customers pay handsomely for access to credit. Regulations also often make it much easier to lend than to take deposits (since the risk rests with the lender).

Saving programs have emerged, and some advocates now claim that deposit services deserve claim to being the most fundamental need for poor families—and for the poorest specifically. But the picture developed by Collins et al (2009) pushes against that view. We argue that a range of financial devices are sought and used together, with different degrees of substitution and complementarity. None has clear primacy.

Programs in locales around the world have experimented with a variety of “credit plus” offerings (notably credit with education) to target poorer populations with mixed results and only recently have a few methodologically sound studies begun reporting evidence. We are just beginning to shine a light on what combination of services, financial and otherwise, will help the poorest build assets and “graduate” into more standard microcredit and other financial products.

One concern is that the optimal mix may not be commercially viable. The costs of building groups, disbursing, collecting and monitoring may overwhelm the return on very small loans at even the highest viable interest rates—and certainly if interest rates are lowered to maximize social impact.

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6. Are borrowing and saving complements or substitutes?

In developed economies, households often use both savings and borrowings to produce large amounts of capital to buy fixed assets like houses and vehicles. House buyers, for example, make a down-payment from their savings and borrow the rest. Saving and borrowing are thus complements in this context.

Behavioral economics provides another mechanism through which saving and borrowing act as complements: for households that are loathe to draw down their hard-earned savings, the ability to borrow—and thus to leave their stash of savings untouched—can function as a helpful way to maintain accumulations. Were households more confident in themselves, or if they had better mechanisms to achieve discipline, “borrowing to save” would be less useful, but in an imperfect world it can be the best of an array of imperfect strategies (Morduch 2010).

In other contexts, borrowing and saving are depicted as alternative activities. In making intertemporal choices, consumers eager to consume today will draw down savings or borrow; consumers focused on consuming at later dates will instead save. This rock of neoclassical theory takes as given that consumers are wage earners, not entrepreneurs (more precisely it takes as given that income is exogenous). For entrepreneurs (like small-scale microcredit borrowers), income is instead endogenous, and they routinely seek to borrow in order to generate income for the future. In this case, borrowing is a forward-looking household activity, not one driven by immediate consumption needs. As Bauer et al (2011) find, it is the more patient villagers in their sample who are most likely to borrow, even more so than their impatient neighbors.

Bauer et al (2011) draw a second connection between loans and saving, arguing that microcredit borrowing can compensate for the lack of disciplined ways to save. Working in villages in Karnataka, India, we suggest that if consumers had better ways to save, they would. But, until then, the microcredit borrowing process can function as an imperfect alternative. The argument is that microcredit allows households to convert small

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amounts of money today into the promise of large future cash flows, just as savings does (Rutherford 2001). In this way, saving and borrowing are substitute activities.

To the extent that this is generalizable, as poor households gain more access to better saving products, will they switch away from credit products? What will this mean for the microcredit business model? Answering those questions requires having a better understanding of how borrowing and saving interact.

7. When and how does financial literacy really matter?

The evidence on financial education has, to date, not been encouraging. As Cole Sampson and Zia write in Chapter 12, being financially *literate* clearly helps, but the value of financial *education* is a different question. We know the desired outcome (literacy) but not a reliable way to get there enough of the time, nor is it clear that literacy is enough. Behavioral economics teaches us that consumers also need ways to implement ideas, especially when temptations and distractions are difficult to keep at bay.

Intuition that improved financial decision making through training would have powerful effects is strong, and there's some evidence in that line. Karlan and Valdivia (2011), for example, randomly selected microcredit borrowers in Peru to receive free business training on issues like cash management, business choice, and marketing. Those who got the training earned greater profits, especially in bad months (at least in some econometric specifications).

But the result from Peru is a bright spot in a landscape in which most studies show little impact. So where exactly are existing financial literacy programs going off track? Is it curriculum? Is it delivery? Is it context?

It's plausible that financial literacy training is most effective when delivered just-in-time, but rarely are financial literacy training programs paired with quality financial products that make consumer choices meaningful. If a person understands compounding interest but cannot gain access to an interest-bearing savings account, the understanding can have little welfare impact.

Still there should be situations where some measure of financial literacy should matter—for instance training on good financial management for

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shopkeepers. Drexler, Fischer and Schoar's (2010) results indicate that impact is possible if we solve the curriculum, delivery and context puzzles. In their case, having simple rules of thumb were particularly valuable. Working with a microfinance institution in the Dominican Republic, they find that providing admonitions to take simple steps like separating business and personal accounts were more powerful than teaching a list of detailed financial concepts.

8. Can the expansion of microfinance add up to macro impacts?

The most basic question is the micro one: whether microfinance typically yields notable impacts on the lives of low-income families. The logical follow-on is, to the extent that micro impacts emerge, how do those impacts add up? Is there a reasonable case that expanding microfinance can make a dent in regional or national economic growth rates? In national-level poverty rates?

There are two complementary research strategies. One is cross-country research, which tends to show positive correlations between financial expansion and the reduction of inequality (Demirgüç-Kunt and Levine 2009 provide an overview). The work doesn't connect the dots from microfinance explicitly, but it does help frame issues. The second approach connects the dots by imposing structure on the relationships. A good example is the general equilibrium analysis of Buera, Kaboski, and Shin (2011). They find that increasing financial access leads to macro impacts, but the magnitudes are small.

The work will be more meaningful as the penetration of finance expands to include more of today's unbanked population. As that happens, it will become more pressing to begin sorting out what this all adds up to.

9. Can increasing access enhance or jeopardize the stability of financial systems?

Regulators hope that expanding financial access will also provide greater stability to the overall financial system. This would occur as the market becomes larger and more diverse, and thus better able to withstand difficulties in any particular corner. The range of depositors would enlarge, as would the kinds of financial institutions in the market. Greater competition among providers would create pressure for quality competition.

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That's the rosy scenario. The financial crisis of 2007-8 in the United States is a contrasting reminder that expanding access and increasing stability do not necessarily go hand in hand. In the United States, the expansion of mortgage finance opened way for new home buyers to engage in speculative and ill-advised real estate investments, eventually fueling the drama behind the financial crisis (McLean and Nocera 2010). The financial crisis was created by a range of forces—including fundamental structural inequalities exacerbated by poor oversight, misaligned incentives, and some measure of outright fraud—so generalization should proceed with caution (Rajan 2010). Still, the crisis underscores the larger point: Regulators need a deeper understanding of what can happen to the stability of financial systems when millions of new participants enter.

Debates over the benefits and risks of commercialized microfinance as a gateway for financial access have raged since the early days of microfinance. Neither the 2010/2011 crisis in Andhra Pradesh, nor prior crises in Nicaragua, Bosnia, Nigeria and other locales have resolved those debates. Clearly there can be a relationship between striving for profitability, fast growth and poor practices leading to overindebtedness of clients and portfolio deterioration.

Yet despite the warning signs, and many public pledges by various industry actors to be working on client protection approaches and social metrics, problems arise regularly. Is there an optimal mix of priorities that protects clients and yet meets the goals for rapid expansion in the number of clients who have access to formal financial services? Is microfinance, like other financial services, likely to experience repeated cycles of boom and bust?

10. What should regulators do?

There's not enough academic research on the regulation of financial inclusion. Many of the questions might seem too applied for some research-minded economists, but that leaves regulators with few guideposts. It also seems short-sighted.

Regulation is always a question of trade-offs between competing goals. Within microfinance, for example, there is evidence that the supervision and monitoring that is part of prudential regulation increases costs substantially for microfinance institutions. That, in turn, appears to push institutions to reduce outreach to their poorer customers and women (Cull, Demirgüç-Kunt, Morduch 2011). The alternative—less regulation in order to increase outreach—carries plenty of dangers. Those are difficult trade-offs to make

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and there is as yet not enough empirical evidence to describe optimal regulatory schemes for microfinance.

Add to this uncertainty the largely uncharted roles of large non-profit institutions. Regulatory schemes are generally designed around reining in the reckless behavior of profit-seeking banks. But non-profits react to regulation differently. Should they, therefore, be regulated by different agencies, those more familiar with the unique behavior of non-profits? Or should they be regulated by finance ministries and central banks like others that provide financial services? Moreover, how do profitable non-profit institutions affect competitive markets?

Answering these kinds of questions will require taking a corporate finance lens and an industrial organization lens to the new, inclusive financial landscapes. Looking forward, these directions will likely yield the most intellectually interesting inquiries of all.

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